DISCUSSION BEFORE THE WIRELESS SECTION, 1ST MAY, 1929.

Mr. E. B. Moullin: I am very pleased to see the use of an aerial in signal measuring work. I have long maintained that many signal measurements, on much longer waves, are made better with aerials than with loops. It may be said that the long-wave measurements are now perfectly simple, but I think it is an advance to use an aerial, more especially for short wave-lengths. I should be glad if the authors would say whether there is any objection to making the connection on the other side of the condenser C_1 in Fig. 2. I do not agree with the authors in their diffidence with regard to the use of a resistance potential divider. I have always advocated the use of a resistance potential divider, as it is more flexible, more simple and more satisfactory than any other method. The check diagrams of Figs. 4 and 5 are very satisfactory. The authors say that the slight bend in the curve in Fig. 5 "is probably due to non-linearity of the detector at low intensities." I cannot see what they mean by this statement, since they are using this method to check the success of their apparatus. Surely they could have calibrated the detector. Is there some difficulty in doing so? With regard to Fig. 7, there is a point of interest on which I should like confirmation from the authors, and that is the extent of the roof of the antenna necessary to make the effective height substantially equal to the geometrical height. On page 146 we have a plain vertical antenna 1·8 metres (5 ft. 6 in.) high. It seems that we must provide 8 ft. radial spokes on the roof before we make the effective height equal to the geometrical height; this means a disc 16 ft. in diameter and only about 5 ft. off the ground. This is a tremendous spread, and I am surprised to find that the curve in Fig. 7 is not more concave towards the axis.

Mr. A. J. Gill: The authors have produced a piece of apparatus which will no doubt be very useful in research measurements on the propagation of waves. In the introduction they mention that in order that the apparatus may be of universal application certain requirements must be fulfilled, one of which is that "The accuracy of the observation should be high, even at the expense of sensitivity." As far as a piece of research apparatus is concerned for work on the propagation of waves, I would agree with that statement; but as far as a tool for the engineer is concerned—for commercial work—I think that sensitivity is probably more important than extreme accuracy, for the reason that when one is using a field-strength measuring set on commercial work one wants to measure stations in commercial use. The signals will probably be varying 100-fold during the course of a few seconds, and whether the accuracy with which these measurements are taken is within 1 per cent or 10 per cent does not seem to me to matter very much so long as the apparatus can cover the whole range. For actual commercial work it must be possible to measure down to fields of about 0·1 μV, so that in order to compete in commercial work the authors will have to increase the sensitivity of their instrument 100 times. They mention that the receiver can be used in a self-oscillating condition, or with reaction. I think that reaction is, however, extremely dangerous to use in any measuring apparatus, the reason being that when reaction is applied to a receiver its sensitivity changes very rapidly and is apt to change during the measurement. About three years ago the Post Office became interested in the measurement of field strengths of short waves, but at that time the only known measuring set for waves down to 15 metres was one made by two engineers at the Bell laboratories.* This set consists of a double detection or superheterodyne receiver having an intermediate frequency of 300 kilocycles and using anode modulation on the first detector. Both detectors contain microammeters in their anode circuits. The first detector is calibrated as a valve voltmeter. The output of the second detector is kept constant by means of an attenuator in front of the intermediate-frequency amplifier. The signal is normally received on a frame aerial, the first detector valve being connected across one half of the frame while a balancing condenser is connected across the other half of the frame, the centre point of the frame being earthed. A powerful local signal oscillator induces a measurable voltage in a small coil which can be connected either in series with the frame or directly between the grid and filament of the first detector. The ratio between the voltage measurements on the first detector under these conditions determines the step-up due to the frame. With the coil in series with the frame the attenuator is adjusted to give a convenient output on the second detector. The signal oscillator is then switched off, the signal to be measured is tuned in and the attenuator is adjusted to give the same output as before. From these attenuator readings and the dimensions of the frame the field strength can be readily computed. In order to measure low fields of the order of 0·1 μV per metre it is necessary to use a vertical aerial. This is calibrated from the frame by measuring, with both, the signal from an oscillator placed a short distance away. Measuring sets having the attenuator between the oscillator and the receiver have a definite limitation in the measurement of very weak fields, due to direct throw-in from the oscillator to the receiver. In these sets this cannot occur and elaborate screening of the local signal oscillator is unnecessary. As an indication of the capabilities of these measuring sets, Fig. A shows the results of measurements taken on a 50-metre tele-

* H. T. FRIIS and E. BRUCE: "A Radio Field Strength Measuring System for Frequencies up to Fifty Megacycles," *Proceedings of the Institute of Radio Engineers*, 1926, vol. 14, p. 507.

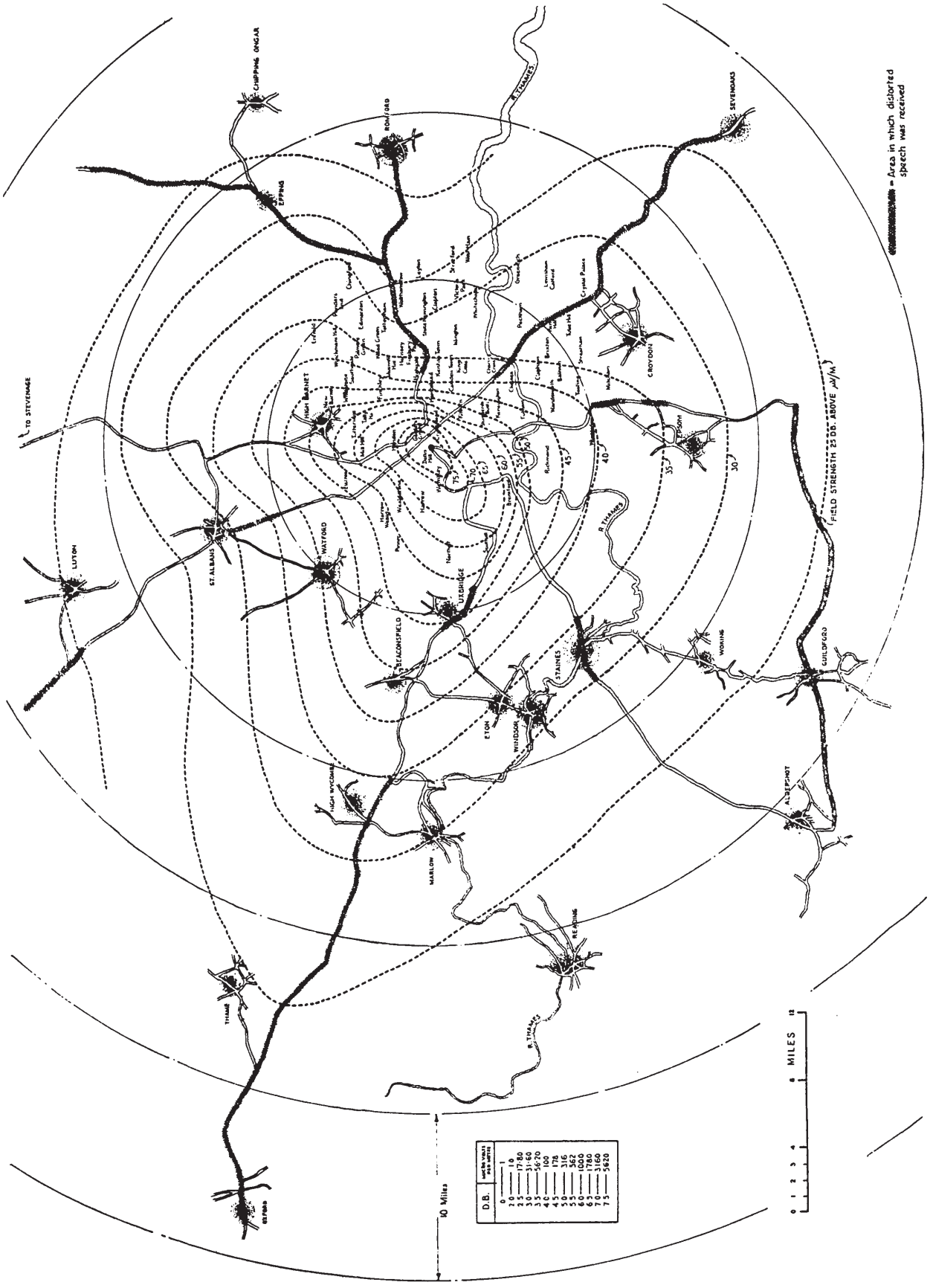
TO STEVENAGE
LUTON
ST. ALBANS
WATFORD
HIGH BARNET
EDGWARE
CHIPPING ONGAR
EPPING
ROMFORD
R. THAMES
SEVENOAKS
CROYDON
UXBRIDGE
BEACONSFIELD
HIGH WYCOMBE
MARLOW
ETON
WINDSOR
STAINES
WOKING
GUILDFORD
ALDERSHOT
READING
THAME
OXFORD
10 Miles
FIELD STRENGTH 25 DB. ABOVE μV/M
= Area in which distorted speech was received
D.B.
MILES

FIG. A.

phone transmitter at Dollis Hill. The radiated power was approximately 500 watts and the contours are drawn for values of fields in decibels above 1 μV per metre. The field distribution near the transmitter was fairly uniform but in the direction of town, and in the east where the land is hilly, attenuation is pronounced. On the country side towards the north and west, however, the attenuation is much less. An interesting feature was the bad quality of modulation which often accompanied fields below 100 μV per metre. This may have been due to frequency modulation at the transmitter. The results represent three days' work with two measuring sets and consist of over 400 separate measurements.

Mr. J. F. Herd: Recent references to short-wave propagation have been plentiful, more plentiful, perhaps, than information on the apparatus or instrumental side of work at these very high frequencies. Consequently this paper forms a valuable contribution to the literature of very-high-frequency instrumentation. I would suggest that more detailed information on the design and construction of the attenuator might profitably be added for the benefit of other workers who may desire it. I am surprised to learn that the authors have been criticized for their aerial arrangement. A very similar aerial system has been in use for some time in work on atmospherics, where we have used an aerial made aperiodic by adequate resistance, with a series resistance in the lead to the grid, in order to render aperiodic the parallel oscillatory circuit which would be formed by the aerial, grid-filament capacity and earth. I cannot agree with the authors' view—implicit in the paper and explicitly stated in its presentation—that the valve shown to the left in Fig. 2 is the first of the system, with the aerial joined to the second valve. The point of aerial entry must surely be the input and the first significant valve of the system, while the left-hand triode is essentially an auxiliary valve for the joint purpose of frequency adretroac tive control. No doubt exactly equivalent results could have been obtained by some conventional form of cascade which would have put this aspect beyond dispute. Incidentally, also, the idea of using a valve for the sole function of retroaction is by no means new, although effected, possibly, by a different circuit scheme from that used by the authors. The gain of 1 to 120 due to adjustment of the local oscillator coupling, mentioned by the authors, is an example of the well-known —but perhaps not sufficiently well-known—phenomenon of optimum heterodyne. The gain quoted is exactly of the order, calculated and measured, which is to be expected with a small signal as the local heterodyne voltage is increased from a low value, comparable with that of the signal, to a higher value which is the optimum for the detector valve in use.

Dr. R. L. Smith-Rose: I have been very impressed by the authors' great change of attitude towards this problem during the progress of their research. About a year ago they believed that it was impossible to make field-strength measurements on these short wave-lengths with any pretence of accuracy. Yet they have shown us a piece of apparatus which not only is efficient, but is apparently almost foolproof: one can touch the aerial and the terminals, which one has been taught never to do even when dealing with much longer wave-lengths. This shows that the apparatus has been brought to quite a high degree of perfection, and I trust that the authors will publish, at a later date, some of the results of its working. I had imagined that this apparatus enabled one to make measurements to an accuracy of about 5 per cent; hence I was surprised to hear the authors apologizing for a discrepancy of less than 4 per cent. I should like to know whether allowance has been made for attenuation in calculating the values given in the table on page 146. Earlier in the paper it is stated that attenuation of the waves over the ground has been ignored because only relative measure-

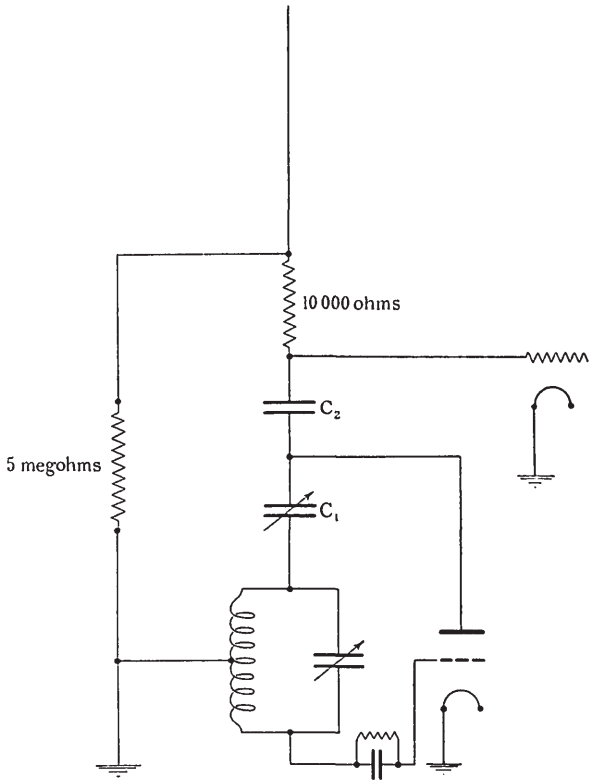

FIG. B.

ments were being dealt with, but in the table the measurements are absolute and I think that, although the distance is only 200 metres (corresponding to about 8 wave-lengths on a 10-megacycle frequency), even for that short range the attenuation is possibly more than 4 per cent, which is the discrepancy in question. I should like to make some remarks about Fig. 2. I do not propose to criticize the working of the apparatus, as I know that the apparatus does function, and I suggest that if one draws out a diagram following on Mr. Herd's arrangement one does get a better idea of what is happening in the aerial circuit. By drawing the circuit diagram in another way (see Fig. B) one can understand that what is being measured is the potential difference generated between the grid end of the tuned circuit and the centre point of the same circuit, i.e. the potential difference across the effective impedance of this half

of the tuned circuit. Since this tuned circuit is connected to a retroactive valve, one would expect that the impedance would vary with the setting of the reaction condenser C_1, which I gather is not the case. I am puzzled by the experiment, described on page 145, of replacing the copper-wire aerial by a eureka wire of 550 ohms resistance, as the result of the experiment appears to be rather unusual. The insertion of a 550 ohms resistance in the aerial of Fig. B would not be expected to affect materially the p.d. being measured. In connection with Fig. 5, the authors made a test for stray e.m.f.'s, the test apparently being to plot the output from the amplifier against the setting of the attenuator, and they say: "If stray e.m.f.'s were present, this curve, if produced back, would cut the intensity axis at a positive point when the attenuation factor was infinity. As will be seen from the curves in Fig. 5, the reverse is the case." Is not this incorrect?

Mr. R. A. Watson Watt (*communicated*): Referring to the very interesting record of rapid fluctuations in intensity of short-wave signals which the authors exhibited at the meeting, I am not sure whether they are to be felicitated or to be sympathized with on taking these first steps in a new and complex branch of wireless meteorology. The record inevitably invites comparison with anemometer records, and the presence of isolated "gusts," together with sudden complete changes in character which would, in the case of wind records, be expressed as a change in the degree of "gustiness," suggests that sufficiently powerful means of analysis will throw light on the turbulence in the regions from which the indirect ray reaches the recording station. But the same considerations of comparison with wind structure suggest that the difficulties of the analysis will not be slight.

Dr. J. Hollingworth and **Mr. R. Naismith** (*in reply*): We are very glad of Mr. Moullin's support of our use of an aerial. Of course it must be borne in mind that for wave analysis, for which this set is ultimately intended, it will probably be necessary to use both aerial and coil, as with a downcoming component in the waves the electromotive forces induced in them are different. The objection to connecting the aerial on the other side of the condenser C_1 is that it would then become an integral part of the oscillating circuit and would sacrifice the advantages gained by its being aperiodic, which is a factor of sufficient importance to offset the loss of sensitivity involved. The primary object of Fig. 5 was to show that the attenuator gave a smooth curve with no appreciable residual electromotive force. This is all that a comparative internal test can do. A second attenuator recently constructed with different electrical and mechanical dimensions appeared on its first test to give a close agreement with these curves; but it had to be returned to the workshops for mechanical alteration, and a complete comparison between the two has not yet been made. The statement as to the non-linearity of the curve is simply an enunciation of the fact that at low inputs to a detector the output current is not proportional to the input electromotive force. The detector was not actually calibrated under working conditions, since the circuit contains inductances and would therefore have to be calibrated at the working audio frequency, for which there was no convenient source available. In this connection it may be noted, in reply to Dr. Smith-Rose's query, that in Fig. 5 there is a small steady current to be allowed for due to the residual emission of the diode with no applied signal. This was accidentally omitted in drawing the graph, and the actual zero line should be about 2 mm higher up in the figure. It is difficult to confirm the statement with regard to aerial spread, except to say that these curves represent actual tests and that in working it is advisable to allow some margin of safety. Only 6 radial wires were used. With a greater number the spread required would of course be less, but the matter was not considered to be of sufficient immediate importance for the tests to be extended in this way.

In so far as the needs of commercial purposes require the reception of very weak signals, Mr. Gill's remarks as to comparative sensitivity are probably true. Two facts must, however, be borne in mind. One is that the limit of sensitivity of our apparatus has by no means been reached, as we are at present using only one stage of amplification and a comparatively large current (10 mA) in the primary of the attenuator. By lowering the voltage of this source so as to decrease this current to 1 mA, and by reducing the 5-ohm resistance in Fig. 3 to 2 ohms, as has been done in the latest model, the minimum electromotive force from the calibrator is reduced to 1/25th of its present value. This is done without materially increasing the trouble due to stray electromotive forces, since the major part of the reduction is made in the voltage of the attenuator primary circuit which is the source of these electromotive forces. The other is that in using the set on the experiments for which it was originally designed our troubles have been rather at the other end, due to the great strength of the signals.

Reaction in a receiver certainly has to be dealt with cautiously. Properly designed, however, the control of it can be made quite smooth, and a very great amount of experimental work has been done in obtaining this condition. At these high frequencies inter-electrode capacities tend to cause excessive "negative" reaction, which requires compensation if reasonable results are to be obtained. While on long waves the ratio $L\omega/R$, which may be regarded as the amplification factor of the oscillating circuit itself (R being the total effective high-frequency resistance of the circuit), can reach a value of 500 without the use of very special precautions, at these frequencies it is doubtful whether even a factor of 10 can be obtained without some form of reaction control.

The stability of the reaction setting has frequently been tested on a local source which may be assumed not to vary; and on occasions the set, after being switched off during the dinner hour, has returned to the same reading immediately it was switched on again. Very slow creep, which may be due to slight frequency change in the heterodyne oscillator, can be allowed for by periodic re-calibration.

In regard to Mr. Herd's suggestion as to further constructional details, at the time of writing the paper only one experimental model was in existence. The first production model, recently completed, gave excellent

results as regards the receiver, but the attenuator required considerable modification. As is pointed out in the paper, the final design must, as always, represent a compromise between various conflicting theoretical requirements, and only by experiment can this be reached satisfactorily. Even now we feel that the time is hardly ripe for a detailed specification.

Dr. Smith-Rose raises the point of accuracy. While the readings given on page 146 are the actual complete results of a test and were not selected in any way, it is probable that they were attended with a certain amount of luck, and we should hesitate without further test to claim invariable accuracy of this order. The attenuation was checked for us by Mr. Barfield. We think Dr. Smith-Rose is wrong in assuming that the apparent aerial impedance does not vary with the setting of the condenser C_1. It probably does, but as this condenser is not altered between reception and calibration this quantity is eliminated in the result.

In connection with the eureka wire experiment, it must be remembered that, although Fig. B represents the theoretical circuit, actually it is impossible to eliminate completely the stray capacities in the 10 000-ohm and 5-megohm resistances, though these have been reduced to the lowest possible value. The circuit would be more truly represented if a small condenser (with a capacity of probably 2 or 3 $\mu\mu$F) were shown across the terminals of the 5-megohm resistance. While this does not affect the calibration of the set, since it is unaltered by the change from " receive " to " calibrate," it reduces the apparent impedance between the aerial terminal and earth and so increases the effect of any resistance added in the aerial proper. Moreover, the replacement of a large part of the fairly thick wire forming the aerial by a piece of No. 48 S.W.G. wire considerably reduces the capacity of the aerial to earth and so adds a further impedance to the circuit. This experiment was actually made on an aerial which passed through the roof of the hut about 5 ft. above the set, an arrangement which has since been rejected as unsatisfactory and replaced by an aerial of $\frac{3}{4}$ in. brass tube strong enough to be self-supporting. However, it achieved its primary purpose, which was to show that the aerial did not form part of a circuit resonant at the frequency of the incoming signal.

It may be well to state that the following fact has played an essential part in the design of this set. The early experiments showed that at these high frequencies it was in places mechanically impossible to reduce strays and residuals to such a value that their effect was strictly negligible. Consequently, in the places where they could not be avoided it was essential to show by extended and critical tests that they could be varied within reasonable limits without upsetting the working of the set. The apparent simplicity of the set is therefore somewhat deceptive, since it is really the result of weeks of careful experiment, testing and selection.

Mr. Watson Watt's remarks will find an echo in the mind of anyone who has attempted to systematize the results of observations on wireless propagation. Even on long waves, which many people now think are fully understood, the ratio of explicable to inexplicable phenomena is painfully low; and the same remark applies with increasing force as the frequency rises. With the large number of presumably independent unknowns present, and the relative scarcity of reliable and comparable observations available, one feels that it would be impossible to accumulate in any reasonable time sufficient data for a strictly statistical analysis. The only hope seems to be a close study of data obtained under as strictly specified conditions as possible, in the hope of obtaining some clues which may enable one to distinguish between what may roughly be described as accidental and essential phenomena, and to vary the line of attack as these observations suggest. While this method is difficult, and by no means infallible, it appears to be the only practicable one; and to carry it out the first requirement is some form of flexible but critical apparatus, of which it is hoped that the set described in this paper may form the nucleus.